REVIEW.

ART. XVII.—*Pneumonia: its supposed Connection, Pathological and Etiological, with Autumnal Fevers; including an Inquiry into the Existence and Morbid Agency of Malaria.* By R. LA ROCHE, M. D., Member of the American Philosophical Society, of the American Medical Association; Fellow of the College of Physicians of Philadelphia; Corresponding Member of the Imperial Academy of Medicine of Paris, etc. etc. Philadelphia: Blanchard & Lea, 1854. 8vo. pp. 502.

WHILE in almost every other department of pathology, our knowledge has, within the last few years, been rapidly progressive, in that of etiology it can scarcely be said to have made any advance since the commencement of the present century.

Facts, it is true, have been accumulated, and divers theories, in relation to the efficient cause of many of the more prominent maladies, have been broached; but when we examine and analyze these facts, we find them to be, often, but carelessly or imperfectly noted, and derived, in the majority of instances, from such limited fields of observation as to form no solid basis for the general deductions attempted to be founded on them.

The subject of etiology is, confessedly, one of great difficulty, and it may be expected to remain involved in doubt and uncertainty until we shall have it in our power to determine the precise conditions under which the several forms of disease are found invariably to prevail. This can only be attained by the collation of full and accurate series of medical statistics, running through many consecutive years; collected amid communities inhabiting the various climates and geological formations of the earth's surface, and studied with especial reference to the condition of each locality—its topography, and meteorological phenomena—the seasons of the year, the ratio of population, the food, clothing, and habitations, together with the political, moral, and social condition and pursuits of the people, as well as the state of the industrial arts in each community.

Since the period when the attention of physicians was first especially directed to the influence of paludal exhalations as a fruitful source of disease, by the work of Lancisi, there has been a constantly increasing tendency with etiologists to ascribe almost all the fevers and febrile affections to a supposed toxical agent, exhaled under the modifying influence of certain contingent influences—heat, humidity, etc.—from organic substances during the process of decomposition. To the morbific agency of the miasm or malaria thus generated has been referred, not only all the forms of what have been termed essential fevers, but also diarrhœa, dysentery, cholera, rheumatism, neuralgia, and even the purely local inflammations of many of the internal organs. There is, in fact, a disposition to constitute this miasm or malaria the one sole cause of nearly all diseases, especially those of an endemic or epidemic character.

"The idea of a close connection, as regards both causation and nature, between thoracic inflammation and malarial fevers of various grades and types, has long been entertained, and continues even now to be advocated by writers of respectable standing. Casually suggested or openly avowed and sustained, at various periods, by professional authorities on the other side of the Atlantic, it has met with especial favour in this country, particularly in our Southern and Southwestern States, where it, at present, enlists many warm and uncompromising defenders."

To demonstrate the fallacy of this singular etiological doctrine is the object of Dr. La Roche in the work before us. To present an abstract of the leading facts that bear upon the question at issue, derived from reliable sources in various sections of the globe, as well as from the results of his own personal observation, and, by a careful analysis of these, to show that the supposition of the identity, etiological and pathological, of pneumonia and autumnal fevers, is founded on insufficient or incorrect data, and is, in fact, little more than a dream of the imagination, is the task assumed; a task which it will be admitted, we think, by all who are open to conviction, has been most satisfactorily accomplished.

In addition to this, his leading object, Dr. La Roche discusses also the question of the existence and morbific agency of malaria in the production of autumnal fevers, with the view to prove that etiologists who regard these fevers as the result of particular poisons floating in the atmosphere of specific localities, have just cause for entertaining that belief.

That the facts adduced in this portion of the work clearly establish the main position assumed by the author—the existence, namely, of a specific aerial poison as the cause of autumnal fevers—will admit of very serious doubt. The leading observations in relation to the general condition of those localities of which fevers are endemic, are it is true very fairly and fully presented. Dr. La Roche has certainly collected a large amount of valuable information in respect to this important subject from the most reliable sources—many of which have never been explored by the mass of the profession. But with all the research he has manifested, and the skill he has displayed in arranging the materials accumulated, we cannot admit that he has succeeded in demonstrating the existence of a specific aerial poison, derived, under the modifying action of certain contingent influences, from organic substances during the process of decomposition, as the morbific agent in the production of autumnal fevers. Admitting the truth of all the leading facts adduced by Dr. La Roche, and we are not prepared to invalidate in the slightest degree either of them, still, the question as to the existence of the supposed specific febrile poison appears to us to remain as open as ever to dispute.

The examination of the connection assumed to exist between pneumonia and autumnal fevers is the subject of the first chapter, and of the last three; while in the second, third, and fourth chapters the question as to the existence and morbid agency of malaria is discussed.

Singular as it may appear, remarks Dr. La Roche, to modern etiologists and pathologists, that the connection as regards both causation and nature, between thoracic inflammations and autumnal fevers should have been made the subject of serious consideration by our forefathers, and still more so, that it should receive the sanction of physicians of the present age, it requires but a slight acquaintance with the medical literature of past and present times to be aware of the fact. To prove this, a few references are made to the writings of well known preceding and contemporary medical authors:—

"The reader will easily perceive," says Dr. La Roche, "that, before the advocates of the identity of the two diseases can successfully sustain the position they have assumed, it will be necessary for them to show that autumnal fevers and pneumonia are produced by the same causes; that they prevail in the same places, and during the same seasons of the year; that their existence and diffusion are promoted by the same agencies; that they are arrested by similar means; that they exercise their effects on the same classes of individuals; that they present similar or kindred symptoms; that they affect the same organs, and produce the same or analogous changes in the fluids and solids; that they are governed by the same laws, and that they present other points of approximation invariably found to be possessed by diseases between which there exists the close connection claimed in the instance before us. Unless they can succeed in attaining these objects, their opinion must fall, and the independence of those diseases be admitted. It becomes necessary, therefore, to take up each of those subjects separately, and to ascertain how far they may be appealed to in respect to the question at issue."

The author adduces a series of observations to show, first: That pneumonia is of common occurrence in situations where fevers seldom or never are seen; 2dly. That pneumonia is not necessarily prevalent where fevers are prevalent; 3dly. That the two diseases prevail in different seasons:—

> "As the name usually affixed to malarial fevers indicates, the latter is, in our latitudes especially, a disease of autumn. It seldom appears before the middle or close of summer, and ceases on the accession of winter. Its existence in an epidemic form in temperate regions, is never known to occur in the spring of the year; cases that occur in that season, or in winter, being accounted for without having recourse to the supposition of the development then of the efficient cause. It never shows itself in winter, and if it appear early in summer—which is seldom the case—the occurrence is only noticed under peculiar circumstances of atmosphere existing during the preceding months, and which invest these with the characteristic conditions appertaining to autumn. It is emphatically a disease of hot weather, requiring for its production a continuance for some time previous of high atmospheric heat. It appears, generally, some weeks after the hottest month; the period being retarded as we proceed north. For the same reasons it may readily be understood to be a disease of hot latitudes, prevailing, as it does, violently and almost perpetually within the tropics, and ceasing long before we reach the polar circle."
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> "If we now turn to pneumonia, we shall find that the period of its prevalence is very different from that of the disease just referred to. Hippocrates some three thousand years ago, and Areteus after him, pointed out the winter as the season in which the disease manifests itself most commonly, and subsequent writers have generally united in that sentiment. Sydenham, it is true, and after him the learned commentator of Boerhaave, affirmed, as the result of their observations, that the disease attains its maximum between spring and summer. But the statement has not always been confirmed by subsequent authorities in this country and Europe."
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> "Everywhere, among adults, children, and old people, idiopathic or primary pneumonia appears to attain the maximum of frequency at the close of winter and in the spring, especially during the months of March and April. Next in point of frequency is the winter; while the disease, though not unknown in summer, is comparatively of rare occurrence during that season, and perhaps still more so during the autumnal months."

4thly, the author shows that the two diseases appear under the influence of opposite winds:—

> "So far as regards malarial fevers," remarks Dr. La Roche, "not a little in this matter will depend on the nature of the surface over which the wind may happen to pass before reaching the locality where the disease prevails; for at the proper period of the year, other things being favourable, fever will appear in connection with any currents which waft the air from neighbouring surfaces where the elaboration of the morbific cause is going on. But, however true it may be, that particular currents of wind exercise a baneful influence in the

> way mentioned, it is not less a fact, which experience will everywhere confirm, that the existence of malarial fever has almost invariably been connected, in temperate regions particularly, with the prevalence, during a greater or shorter space of time, of southerly currents, and that the influence that these exercise is not necessarily, and in some forms of the disease is very seldom dependent on the malarial nature of the localities over which they happen to pass. Nor could this well be otherwise, for these fevers require for their development the long continuance of a range of thermometrical heat, which could seldom be obtained during the prevalence of opposite currents. Now, how do matters stand in that respect as regards pneumonia? If in some localities, as at Gibraltar, for example, the disease prevails most usually during those periods of the year when westerly winds are predominant—if the disease, also, is found to occur more frequently in other places under the influence of different currents, it may be laid down as a general rule, that north, northeast, and northwest winds are those during the prevalence of which thoracic inflammations are more frequently developed. Such was found to be the case in the north of Italy and Germany by J. Frank. Le Pecque de la Cloture made similar observations in Normandy, as did also Hourmann and Dechambre, and Grisolle at Paris." "Similar results have been noted in England, in Nova Scotia, and Bermuda; and it can scarcely be necessary to add that in this country pneumonia is almost invariably associated with the prevalence of northerly currents—N. N. E., N. W., or west."

It is shown, 5thly. That while pneumonia is of yearly occurrence, this is not always the case with fevers even in those localities of which they are the especial endemic; 6thly. That the altitudinal range of the two diseases is not the same—autumnal fevers being the endemic of valleys, plane surfaces, the banks of streams, lakes, and other depressed localities, while they very commonly spare high and well-aired situations. On the other hand, pneumonia, though not a stranger to low grounds, valleys, and similar situations, prevails as widely, if not more so, on mountains, hills, and other elevated places. 7thly. That fevers are influenced by the nature of the soil, which is not the case with pneumonia. 8thly. That fevers are arrested by frost, but not so pneumonia—this being even increased in point of frequency by the very thermometric change which puts a stop to the occurrence of autumnal fevers.

It would be impossible to present a satisfactory analysis of the numerous observations adduced by Dr. La Roche from the professional experience of the medical writers, as well of the past as of the present time, in support of each of the above propositions. These observations are in general well selected and apposite. Occasionally the author may, it is true, be accused of having overloaded the subjects discussed by the multiplicity of his references; nevertheless, all the facts he has enlisted in the argument are replete with interest, and deserve a careful perusal and candid consideration on the part of all who desire to judge clearly of the differential etiology of what have been denominated the essential fevers and pulmonary inflammations.

Passing by for the present the three chapters devoted to the subject of malaria, we arrive at the fifth chapter of the treatise. In this, pneumonia and autumnal fevers are compared in reference to their causes, mode of progression, symptoms, anatomical characters, and the circumstances by which they are influenced.

The difference in the causes by which the two diseases are respectively produced is clearly indicated.

> "We have seen," says Dr. La Roche, "in a preceding chapter, that the prevalence of malarial fevers, of various grades and types, is restricted within certain localities; that in many instances, the area of those infected places is very limited in extent; that while the disease prevails in one spot, individuals who

reside at a short distance, and abstain from visiting that spot, escape; that by removing from one part of the same city to another not far distant, or from one end or side of a house to another, or from a lower to a higher story, the disease may be avoided; that ships, by shifting their position from one part of a sickly port or shore to another close by, are often found to lose the fever, from which, before the change, they had suffered severely. We have seen, on the other hand, that in certain localities, the crews of vessels that had been exempt from fever so long as they remained at only a short distance from land, were attacked, sometimes to a man, as soon as they ventured ashore; and that individuals who had enjoyed good health while avoiding infected city or country localities, were attacked with an almost unerring certainty, in consequence of visiting or passing through them. Now, the physician who should undertake to collect facts to prove that the sphere of prevalence of pneumonia is as narrowly circumscribed as it is sometimes found to be in fever, would have an ungrateful task to perform. We nowhere hear of this disease attacking a large number of the residents of a limited spot, of a part of a house or street, and leaving every one in the close vicinity of that spot, in the next street, or in the adjoining houses, perfectly unscathed. Nor need we fear to predict that the medical writer who ventures on the assertion that pneumonia has frequently been observed to attack the occupants of the lower rooms of houses, or the basement wards of an hospital, and to scrupulously respect those who dwell above; that all the inmates of one end of an asylum, hospital, prison or house, have been struck down by inflammation of the lungs, while those occupying the other parts of the same building have remained untouched—and that, too, not during one season, but during a succession of seasons—will run great risk of giving but an unfavourable opinion of the authenticity of his facts, or the soundness of his judgment. We do not hear of the crews of ships ridding themselves of the disease in question, by shifting their position, and anchoring at the distance of a few dozen yards; or of vessels, which before had been healthy, becoming by a reverse change, or the removal of an intervening ship or other object, suddenly visited with the disease. Neither do we find a large number of instances on record of vessels which were free so long as they remained under sail, or at anchor at a short distance from an infected shore, being filled with pneumonic cases the moment they approached close to the land, or sent their boats to explore the mouths of rivers, &c.; or, again, of scores of individuals, who had remained healthy while residing at a short distance, being attacked with pleurisy or pneumonia in consequence of jumping over the barricades, and promenading about the streets of an infected spot."

After referring to the fact, that those means by which the condition of a locality may be so far changed as to put a stop to the prevalence there of fevers have no such salutary influence in respect to pneumonia, Dr. La Roche proceeds to point out the difference there exists between the distinctive symptoms and pathological conditions of pneumonia and malarial fevers. This difference, showing that, if we admit an intimate relationship, in cause and nature, to exist between the two diseases, we must admit, also, that from one and the same morbific agency are produced diseases differing widely in their symptoms, march, and anatomical characters.

In thus indicating the general purport of this portion of the essay before us, we have, we admit, scarcely done justice to the manner in which the author has treated the several important particulars in reference to the etiology and pathology of autumnal fevers and of pneumonia that are discussed in it. Our aim has simply been to indicate to our readers the general scope of the author's arguments to disprove the presumed identity between autumnal fevers and thoracic inflammations. The entire chapter is deserving of a careful perusal, comprising, as it does, much valuable matter, collected with commendable industry, and faithfully and clearly presented. In the section which treats of the causes of pneumonia will be found a series of interesting observations in relation to the particular conditions under which inflammations of the thoracic organs are most liable to occur.

In the sixth chapter, pneumonia and autumnal fevers are compared in reference to the power of acclimatization and the ages, sexes, and races of those most subject to be attacked.

Dr. La Roche shows that the exemptive power of acclimatization does not extend to pneumonia, though well marked in respect to the various forms and grades of autumnal fevers.

In regard to the liability of different races to the two diseases, it is shown, that while the negro is, to a greater or less extent, exempt from autumnal fevers, he is as subject as the white to attacks of pneumonia, if, in fact, not more so.

> "We are scarcely less justified," Dr. La Roche remarks, "in seeking for proof of dissimilarity between the two diseases in the difference of liability to each of the two sexes. As regards pneumonia, males may furnish generally a larger number of cases than females, and the disease in them may assume frequently its most severe character. But there are facts sufficient on record to warrant the conclusion, that this greater prevalence of the disease in the first-mentioned sex is not the result of an inherent susceptibility, but is due, when it occurs, to a series of fortuitous and modifying causes, more particularly to the circumstance that males—owing to the nature of their avocations and mode of life—are usually more exposed than females to the causes of pulmonary inflammation; and that in places where exposure is equal in both sexes, the disease manifests itself as frequently in one as in the other. On this subject, statements, for which we are indebted to Grissolle, Chomel, Williams, Valliex, and others, can leave no doubt on the mind of the unbiased inquirer, so far as regards Europe. For, besides that, in rural districts, where women are as much exposed as men, the disease does not manifest itself more frequently in one sex than in the other; in prisons, for example, where the material conditions of life are similar for all the inmates, the number of females attacked equals that of males. Nor should it be forgotten, that among children, who are exposed to the same influences, the disease has usually been found to bear with equal severity on the two sexes. Similar observations have been made in this country and elsewhere.
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> "In regard to malarial fevers of various grades or varieties, from the simple intermittent to the deadly and malignant yellow fever, we arrive at different conclusions. In these males, excepting in some epidemics mentioned by Musgrave, Catel, Rufz, and under circumstances of a special kind, are more frequently affected than females; and, as a general rule, it may be stated that, when these are attacked, they have the disease in a milder form. That this comparative immunity on the part of females may, in some measure, be due to their more temperate habits, and to their being usually less exposed to the deleterious influence of night air, or, perhaps, as is presumed by Copland, to the state of the female constitution, during the period of uterine activity, is doubtless true; but it is equally certain, that, after making every possible allowance for the efficiency of these causes of resistance, we still find enough to convince us that females are far less obnoxious to the impression of the febrile poison than individuals of the other sex."

A difference of susceptibility to the two diseases, at different periods of life, is next adverted to. Thus, while the young and old are comparatively little amenable to the influence of the causes productive of autumnal and periodic fevers, pneumonia is in them a very frequent disease.

The well-known effects of the depressing passions and emotions, as well as of anger and of other of the exciting ones, as predisposing and exciting causes of fever, and their inoperativeness in the production of an attack of pneumonia is pointed out as another evidence of the difference between the two diseases.

The prevalence of fevers and pneumonia at the same time, and in rapid succession, upon which much stress has been laid as an evidence of their intimate relationship to each other, furnishes, as Dr. La Roche shows, no support to the doctrine attempted to be based upon it.

> "Already, in the opening chapter," he remarks, "attention was partially called to this circumstance, and enough was perhaps then said to justify the inference, that nothing favourable to the idea of the identity of pneumonia with periodic fevers could be made out of the fact that the two diseases coexist, or that the former succeeds to the latter. It was there remarked that inflammation of the lungs prevails very extensively in places where remittent, intermittent, and other fevers of kindred nature are not observed, that it shows itself usually at seasons of the year when, if the ordinary causes of fever had at any time exercised their influence, they have been effectually or temporarily removed; that in places where periodic fevers prevail during a certain period of the year, they are put a stop to, in all their varieties, by frost. It was shown that while fevers are thus arrested, pneumonia, which had coexisted with them, instead of disappearing also, continues to prevail as it did before the accession of frost, or even is observed to spread more extensively; and that as the cause of the fever had thus been destroyed—as proved by the entire absence of its legitimate effects—the cases of pneumonia which continue to show themselves after the accession of frost, cannot be referred to the morbid agency of the cause in question, but are due to the operation of some other morbific influence, over which frost exercises no control, and differing, consequently, from the former. It was stated that the same causes which give rise to pneumonia *after* a stop has been put to periodic fevers by frost, must be similar to those that produce the disease *during* the prevalence of those fevers and anteriorly to the occurrence of frost. It was, moreover, argued that, if the cause which produces pneumonia *after* that event must, for reasons stated, differ essentially from that occasioning the fevers thus arrested in their course, the cause giving rise to the cases that appear *during* the fever season, must also be different from the febrile poison; that hence, when the two diseases show themselves together, two sets of causes are at work; that from this difference of cause we have reason to infer the existence of a difference in the nature of the diseases produced; and that when pneumonia, in the regular succession of the seasons, follows on periodic fevers, after the accession of cold or frost, or at the period of atmospheric vicissitudes, it is not influenced in its production by the cause of those fevers. It appears, therefore, as the effect not of a gradual change from one form to another of the same complaint, but as the result of the creation of a different, or the continuance of an independent disease."

The chapter closes with a reference to the non-convertibility of pneumonia and autumnal fevers.

To understand the full force of the argument adduced from the non-convertibility of the two diseases, it is necessary to have a clear understanding of what is understood by the term convertible diseases. Is it intended to convey the idea of an actual change of one malady into another? or of the modification, in the course of a disease, of certain of its phenomena and the development of others, in consequence of which its characteristics become changed? or, is all that is meant merely the change *from* one disease *to* another?

The fact of the coexistence and succession of any two diseases furnish no evidence of their convertibility and consequent identity in the two first-mentioned senses. Fevers of an intermittent and remittent type may with propriety be adduced as examples of truly convertible diseases.

> "Under much variety of aspect, as is remarked by a most eminent writer of this country (Dr. Drake), these fevers 'possess many deep-seated analogies and identities; they frequently change from one type to the other. Thus, an intermittent turns into a remittent, and the latter, assuming the type of the former,

is often seen to become, first a quotidian, then a tertian, and finally, a quartan. A simple intermittent may, in the third or fourth paroxysm, take on the character of a fatal congestion; and that which began with an aspect of malignity, sometimes emerges into simplicity and mildness; vernal agues attack those who, in autumn, had suffered under remittent fever, not less than those who had experienced the intermittent form; the sequelæ of all the varieties are almost identical: the same treatment, with certain modifications, is applicable to the whole.' Surely," Dr. La Roche remarks, "nothing of this kind is observed to occur in reference to many diseases, the identity of which is insisted upon on the ground that they coexist together, or follow each other in the same locality. The yellow and common autumnal fevers, though kindred zymotic diseases, and arising from malarial exhalations, are not, strictly speaking, convertible, though cases occur which exhibit symptoms characteristic of both, and the diseases may either blend together, or appear in rapid succession in the same subject. Remittents or intermittents never change into yellow fever, and *vice versâ;* their anatomical characters and sequelæ are not the same; those attacked with yellow fever in the autumn, are not affected with ague the following spring, and the aspect of the two diseases are not the same. In a word, they do not possess deep-seated analogies and identities similar to those existing between the various forms of ordinary paludal fevers. Still less allied to autumnal fevers in those respects, are typhoid or typhus fevers, and true Oriental plague, which, under the fostering hands of some unitarian pathologists, have been admitted into the family of periodic marsh miasmatic fevers, there to keep company with yellow fever, Asiatic cholera, typhoid pneumonia, to say nothing of phrenitis, gastritis, gastro-enteritis, hepatitis, and, for aught I know, peritonitis, cystitis, tonsillitis, nephritis, gout, rheumatism, and the rest of the forty or fifty different varieties of diseases into which, as we are told, the nosology of southern fevers might be arranged, and which constitute so many links in the chain of morbid action, extending from a septenary ague up to the most violent and fatal form of yellow fever. At a still greater distance, in these same respects, from autumnal fevers do we find pneumonia. So far as I am aware, the physician is yet to be found who has discovered that pneumonia and periodic fevers are convertible diseases in the way that the several forms of these have been shown to be. A case of intermittent or remittent is not converted into pneumonia by injudicious treatment, or a case of pneumonia transformed into a mild intermittent by proper, or into malignant remittent by improper remedies; their sequelæ are not identical. Those who have pneumonia in the autumn or winter, do not run as much risk of suffering from vernal agues or summer remittents, as those who have passed through these complaints; they do not possess many deep-seated analogies and identities, and the same treatment would require more than trifling modifications to make it applicable to both.

"In saying this much on the subject," Dr. La Roche adds, "I am far from denying the change from one disease to another. Such changes are of daily occurrence, and are observed in regard to almost every complaint to which the human system is subject. A case commences with symptoms of common remittent or intermittent fever, and at its close exhibits phenomena appertaining to yellow fever. In other instances the reverse occurs, cases of yellow fever ending with symptoms of periodic fever. Typhus, typhoid, or pestilential fevers terminate sometimes in the same way; while, at other times, cases which at their outset presented the characteristics of common intermittent or remittent fever, assume, as the disease progresses, those of the fevers mentioned. So also with regard to pneumonia and fever. Cases of the former not unfrequently, under peculiar endemic or epidemic influences, end with symptoms of autumnal, as also of yellow, or typhus, or typhoid fever. On the other hand, cases of periodic or other fevers sometimes terminate with symptoms of pneumonia.

"Doubtless changes of the kind may, strictly speaking, be regarded as cases of conversion; but the conversion herein noticed is not that of one form of a disease into another form of the same. It cannot be occasioned by an increased force in the cause of the disease first existing, by a difference in the state of predisposition of the person attacked, or by the peculiar mode of treatment pursued. It is not the result of a mere modification of one and the same thing; in a word, it is the substitution, partial or complete, of one disease for another. Such conversions of diseases are not of rare occurrence. They are, indeed, familiar to all practitioners. They often lead to evil or fatal consequences, or simply to the removal of disease without restoration to health."

The seventh and concluding chapter treats of the complication of pneumonia with autumnal fevers. This complication of the two diseases forming a hybrid affection, which must not be considered as a peculiar form of either. It is admitted that in miasmatic regions the complication alluded to frequently takes place, causing pneumonia there—like very many other diseases—to assume, to a greater or less extent, the periodic type. The pneumonic symptoms and the malarial fever are concomitant diseases in the same individual, and affect the patient according to the type of the latter; in paroxysms, if it is intermittent, in remissions and exacerbations, if it is remittent; that is, the pneumonic symptoms are always exasperated during the presence of the fever, and mitigated during its intermission or remission. In the combination of phenomena exhibited in the cases referred to, of pulmonary inflammation and periodic fever, Dr. La Roche maintains that we are not justified in assuming the identity of the two diseases, or that the one is a modification, or really and substantially nothing more than a peculiar form of the other. To him, as to some of the writers he refers to, and to many more that might be cited, all such cases furnish illustrations of the coexistence of two distinct complaints, produced by distinct causes, having distinct seats and characters, and governed by different laws, but which often modify each other to a greater or less extent.

The complication of diseases more or less distinct in their nature, and the modifying influence of epidemic over other complaints, Dr. La Roche shows to be facts well known to etiologists and pathologists. He also refers to the fact that diseases arising from various species of malaria, but marked by distinct characters, and governed by different laws, combine with each other, and present groups of phenomena which, though they have given rise to considerable discussion, and been subjects of angry controversy, must be viewed as the effect of such complications, and not as mere modifications of one and the same disease. Thus, diseases due to specific contagious poisons amalgamate together, or with other complaints, and form hybrid complaints, or exist together in the same subject.

Dr. La Roche admits that pneumonia, like other inflammations, sometimes assumes a periodic type, independently of a malarial influence. Of this there can be no doubt. The manifestation of a periodic character, he insists, can lend no support to the idea of identity, as regards causation and nature, of pneumonia with malarial fevers, of which periodicity, partial or complete, constitutes a characteristic element; unless we are prepared to assert that all diseases in which we notice perfect or imperfect remissions—whether observed in districts of country subject to malarial complaints, or in places totally free from these—are the products of the same cause as remittent and intermittent fevers, and consequently really and substantially nothing more than peculiar forms of them. From numerous facts upon record, and the legitimate deduction from these, it will admit of a reasonable doubt whether the element of periodicity does really belong exclusively to febrile diseases of a malarial origin.

"So far from it," remarks Dr. La Roche, "the periodical is as much a natural type as the continued. It characterizes many of the phenomena of health, and exhibits itself in the physiological play—both as regards progress and intensity—of many of the functions; in the processes of secretion, elimination, and calorification; in the operations of the nervous system; in muscular contraction; in the action of the heart, &c. Intermittence, indeed, may well be viewed as an element essential to the existence of the normal actions of the economy. What is more, it adheres to these actions in their passage from the state of health to that of disease; and may, therefore, be recognized as an element of this state also. It stands as an illustration of the great law of periodicity which regulates all the vital movements."

Dr. La Roche next shows that the success of the anti-periodic treatment in pneumonia—supposing it true—is no proof of the identity of the disease with periodic fevers. In the course of this section doubts are expressed, and, we believe, well-founded ones too, as to the propriety and success of the abortive plan of treating febrile diseases, by means of quinia, given in scruple or even larger doses, with or without preparation, at the outset of the attack, with the view of arresting their progress.

The chapter closes with a short recapitulation of the reasons for believing that the hypothesis of the identity of pneumonia with autumnal fever is not supported by facts and solid arguments.

The work of Dr. La Roche is deserving of an attentive perusal on the part of every one who would make himself master of the facts and observations bearing directly upon the important etiological and pathological subjects discussed by its author, as they are recorded by the best medical authorities of the present and former times. These will be found interesting and instructive not only as they throw light upon the question of the supposed identity of pneumonia and autumnal fevers, but, also, in reference to many collateral points of deep interest and importance, bearing upon the causes and character of the diseases referred to. Many of them, also, derived from medical works of somewhat difficult access to the mass of the profession. The very profusion with which Dr. La Roche accumulates from every reliable source, facts to sustain each limb of the argument levelled by him at what he believes to be erroneous views of the etiology and nature of pulmonary inflammation, although entertained by writers of no mean authority in our profession; his desire to impress on the mind of his readers an idea of the appositeness and authenticity of these facts, and to enable them to verify their accuracy, by presenting the facts mostly in the language of their reporters, and pointing out where the record of them is to be found, constitute in our estimation no trifling recommendation of the work before us, and materially enhance its value and interest. The patient and laborious research of the author thus placing within the reach of every physician the results of the medical experience and observations of the past and the present; saving him the time, the labour, and the expense of consulting many books to learn what is actually known in reference to the important questions of which he treats—what views have been already shown to be erroneous, and upon what points we still remain in ignorance.

Let us now direct our attention to the author's views in relation to the existence and morbid agency of malaria.

In regard to the objections that have been urged to the existence of malaria as a cause of fever, Dr. La Roche maintains that the appearance of the disease in localities where no marshes exist does not disprove the agency of a poisonous exhalation existing in the atmosphere:—

"The writings," he remarks, "of Chervin, Boudin, Nepple, Maillot, Segond, Faure, &c., which are usually referred to in support of the strictly paludal origin of fevers generally, will show that, even in the opinion of these authors, malarial exhalations of various degrees of virulence may, and do often proceed from surfaces presenting characters very different from those appertaining to ordinary marshes. Indeed, at the present day, this existence of malarial exhalations, and their efficiency in the production of fever, independently of the presence of marshes, properly so called, and their elimination from sources of various nature, and differing much in external appearance, is almost universally admitted—quite so, I think, by all who have taken pains to investigate the subject in all its bearings; for, though they are firmly convinced of the reality of the morbific agency of such effluvia, they know that fevers prevail sometimes even in arid places with want of surface-water, where the soil is rocky, or sandy, parched, and deficient in vegetation, and where, in a word, circumstances generally are, in appearance at least, unfavourable to the decay of organic matter. On this subject, the facts recorded by Ferguson, J. Davy, Craigie, Brown, Currie, Humboldt, and others, can leave no doubt. Nay more, it is almost as generally acknowledged, that the malignant forms of such diseases are never produced by the effluvia of genuine marshes, but are the products of other miasmal sources; while, on the contrary, fevers known to arise from marsh exhalations, are never produced by the effluvia which occasion the other forms of the disease. Hence, when ordinary or malignant autumnal fevers occur in places where no marshes properly so called exist, it is of no avail to cite the absence of these as an evidence of erroneous conclusions; far less of absurdity, on the part of those who attribute such fevers to miasmatic exhalations. The latter writers know, fully as well as their opponents, that the existence of a marsh is not indispensable to the manifestations of the effect in question; but, unlike them, they are perfectly aware of the fact that morbific effluvia, of the most deadly character, too, may, and do arise from sources which bear no resemblance to a marsh. In the words of an intelligent writer, we may say: 'Marshes and swamps are far from being the only sources of miasmata. The foul shores of the sea, the moist slime and mud of the banks of great rivers, and of mill ponds; the mire and mud of the unpaved streets, ditches, lanes, and passages of great towns and cities, villages, &c., particularly the cellars and damp abodes where the poorer classes are most frequently doomed to dwell—the moats of garrisons, &c.; the soil where certain hospitals, barracks, or encampments are situated; the wells and cellars, damp pools and dungeons of prisons, and the holds of ships, are all calculated to emit pyrexial effluvia from the moist earth, mud, and filth, which are mostly to be found within their precincts.'"

The non-detection of malaria in the atmosphere of insalubrious localities is, Dr. La Roche insists, no evidence of its non-existence and morbific agency. Chemists, as he remarks, have not been more successful in the discovery of the poison of other zymotic diseases, smallpox, scarlatina, measles, hooping-cough, &c., and yet we know that they must at times float in the air, since they produce their respective morbid effects in individuals who breathe that tainted medium; and under circumstances, too, which forbid the supposition that those attacked could have received the infection by direct exposure to, or contact with, the sick.

Dr. La Roche then proceeds to show that fever is not due to the morbific action of any of the known gases floating in the atmosphere, or to an excess or deficiency of one or more of the known constituents of the latter in unhealthy localities. But, while admitting the truth of this, we can find, he remarks, no valid reason for denying the very existence of a specific febrific cause; for, from the circumstance that malaria cannot justly be identified with any of the known gases, it does not follow that the atmosphere of sickly localities contains no extraneous material to which autumnal fevers are to be ascribed. Some statements are then referred to, in reference to certain azotized flocculi, said to have been detected in the moisture of the atmosphere, and the dews of insalubrious districts, by some experiments performed in the early part of the present century. These statements are deserving, however, of but little consideration. They do not of themselves, unsupported as they are by any extended series of observations, afford any evidence that the azotized matter referred to, even admitting its frequent existence, constitutes the febrific poison. It has been detected in air issuing from noted sources of vegetable and animal putrefactions, and which, nevertheless, did not give rise to malarial fevers, while, on the other hand, chemists have not unfrequently failed to detect azotized flakes in the air of localities where fever prevails more or less extensively.

Dr. La Roche infers the existence of malaria as the morbific agent in the production of fevers, from the character and condition of the localities in which these diseases prevail. Of the localities referred to, he presents a very full and accurate description.

The danger of an attack of fever, he shows, increases in proportion to proximity to such locations. If we approach to, or remain some length of time—occasionally only a few hours or even moments—in them, or in their immediate vicinity, we are stricken down with fever; if we avoid them, we escape.

A series of facts are adduced to prove that the morbific influence of these localities does not result from heat alone, nor from humidity, nor from a high dew-point, nor from heat and humidity combined, nor from vicissitudes of temperature, nor, finally, from a peculiar electrical condition of the atmosphere. We are not prepared to attack the correctness of either of the positions just enumerated—we admit that, of itself, neither of the circumstances referred to, is sufficient to account for the production of fevers in the localities where these prevail; we cannot, nevertheless, perceive that we are hence necessarily forced to admit the existence of a specific aerial poison, emanating—under the modifying influence of certain contingent influences—from organic substances during the process of decomposition. To the term malaria, however, when employed merely to express the morbific state or influence of the atmosphere of certain localities, whatever may be its precise nature and cause, there can be but little objection.

The innocuousness of some marshes, and of localities similar to those that are sickly, Dr. La Roche proceeds to show, are not evidence of the non-existence or non-agency of malaria. The exemption, he remarks, may be explained in various ways:—

> "Sometimes it is due to the high elevation above the level of the sea of the places so exempted. At another time, the effect is attributable to the absence of a sufficiently high and long-continued atmospheric heat. In other instances, the circumstance is due to a very perfect and constant ventilation, and a very rarified and pure character of atmosphere. In some instances, again, it may be explained by the peculiar geological character of the soil; the quantity and the quality of the surface water; or the proportion of sulphates the latter contains in solution. Sometimes, also, it is due to the rapidity of the river currents; the excessive and rapid dryness of the atmosphere during the hot season; the existence and extensive prevalence of refreshing and purifying winds, and often to the degree of desiccation the surface has attained by natural or artificial means; the degree of cultivation to which it has been carried, and other agencies of like import, as well as by the extent to which it is sheltered, by rich foliage and other means, from the action of the sun. So far as ships are concerned, the freedom from fever will often be found ascribable to the latitude in which they may be navigating; to the early period of the year in which they may be at sea, or otherwise employed; or to the absence of an epidemic constitution of atmosphere."

The leading causes of exemption from fever of certain localities, whose general characteristics would lead to the inference of their being malarial, are separately considered by Dr. La Roche; and, with his characteristic industry, he has brought forward in reference to each a large amount of facts to prove and illustrate its direct bearing upon the question under discussion.

The ensuing two chapters are principally occupied with an exposition of the more prominent of the facts which, in the estimation of the author, establish the existence of malaria, and its agency in the production of disease. In these chapters will be found an excellent digest of nearly all the important observations upon record illustrative of the conditions and localities favourable to the production of autumnal fevers, and of the means by which those conditions may be modified or changed, and the insalubrity of the locations abated or destroyed.

We cannot follow Dr. La Roche throughout the several branches of his certainly most able argument in favour of the existence of a specific aerial poison as the cause of autumnal fever; nor can we consider in detail the various facts adduced by him in support of his general deductions. Of the accuracy of these facts, there can be no doubt; how far they bear out the author in his views respecting the nature and source of malaria, we leave to the readers of the work before us to decide.

That we may do entire justice to the views of Dr. La Roche in regard to the etiology of autumnal fevers, and the arguments by which they are sustained, we present the twenty-six propositions that he believes to be legitimate deductions from all the facts and statements passed in review by him.

"1. The doctrine of malaria, though of ancient origin, and very generally admitted, has encountered, and continues to encounter, opposition.

"2. The appearance of autumnal or periodic fevers, where there are no marshes, properly speaking, does not disprove the existence or agency of malaria in the production of that class of disease, inasmuch as there is nothing to hinder morbid exhalations from being furnished by terrestrial surfaces of a different character, and no writer of any reputation has denied the fact of such occurrences.

"3. The constant association of these fevers with peculiar characters and conditions of localities, and their absence or cessation where these characters and conditions do not exist, or, having existed, have ceased to do so, through the operation of artificial or other means, lead to the opinion of the evolvement from these localities of some peculiar morbific poisonous substance from the soil, or the materials by which it is covered; and of the connection, as cause and effect, between this exhaled substance and the disease in question.

"4. The inability of the chemist to detect this malarial poison in the atmosphere of sickly localities, and to point out its nature, does not disprove its existence, inasmuch as other substances, the presence of which cannot be doubted, equally escape detection.

"5. Atmospheric heat alone will not serve to account for the production of periodic fevers of various grades and types; for these fail to appear in seasons when, and in places where, the thermometer ranges higher than at other seasons when, and in places where, they prevail extensively. Besides, instances are not rare when fevers have stopped though the heat continued unchanged, and apparently from the influence of a very high range of temperature.

"6. Nor can terrestrial or visible atmospheric humidity account alone for the effect under consideration. Though fevers often break out or prevail during wet weather, they usually cease when this humidity is at its height, and reign most generally during the drying process—often during very dry spells of weather.

"7. Fevers of the kind mentioned cannot be due simply to a high dew-point, for they exist and are rife when the latter is not higher than in healthy seasons.

"8. For reasons assigned, the efficient cause of fever cannot be sought in any peculiar modification in the electrical state of the atmosphere. This fluid, by its excess or deficiency, may and does, no doubt, exercise an agency in the matter; but that agency is evidently limited to placing the system in a proper condition to receive the impress of a morbific cause; for furthering, when deficient, the formation of the latter, or, when in excess, to neutralizing or destroying its effects. But, in all cases, the presence of such a cause is required before autumnal fevers can be produced.

"9. The same may be said of atmospheric vicissitudes. These may and do often occur, without periodic fever being the consequence; and, conversely, those fevers frequently occur without appreciable vicissitudes.

"10. The attack comes on too suddenly, and, not unfrequently, after too transient an exposure to evident sources of infection, without the possible occurrence of any other influencing agency; at other times, it occurs too long after a residence in, or visit to, places where the disease is known to prevail, to be the effect of any other cause than a morbid poison introduced into the system.

"11. The opinion of the existence and agency of such a poison will appear the more natural when we find that the danger of an attack is generally proportionate to the proximity to localities where these diseases prevail—the other agencies being the same beyond as within the sphere of their prevalence; that vessels on a sickly coast remain healthy so long as they do not approach the land; that they become again healthy by removing to a short distance, or by merely shifting their position; and that individuals from on board, who land, and those especially who sleep on shore, are almost certain of being attacked.

"12. The development and prevalence of fever on board of ships, when other vessels in the vicinity remain healthy; its occurrence only in limited parts of the under decks; and its being arrested by a proper system of expurgation, lead to the opinion of the generation and existence there of a morbific poison.

"13. The innocuousness of some marshes, of ships in a foul state, or of surfaces bearing a strong resemblance to others in which periodic fevers prevail, is no proof that exhalations issuing from sickly places or ships are not the cause of the disease; for, besides that the latter is often traced to some source of decomposition in a way to leave no doubt as to the agency of these, the exemption may be justly attributed to the existence, in some instances, or to the absence, in others, of a variety of necessary and concomitant circumstances. Fevers do not prevail beyond a certain altitudinal range; they require a certain amount of heat, and that this heat should be continued during a certain length of time; they require, besides, a certain amount of terrestrial humidity, no more nor less; or, at least, that the soil should have been well saturated before being exposed to the prolonged operation of the solar heat; also, a close and still atmosphere, and other contingencies adverted to. Without these, they do not appear to show themselves, how favourable soever to the generation may be the condition and nature of the soil, or the substances by which it is covered or permeated.

"14. The malarial doctrine receives further support from the well-attested fact, that the cause of fever is carried by the wind from paludal and sickly localities to places situate at considerable distances; the latter places remaining healthy as long as they are to the windward of the others, and becoming sickly when they are to leeward. In such cases, the wind cannot have carried an amount of heat or humidity sufficiently different from that existing before to account for the effect produced. Nor can it act by occasioning a much greater amount than usual of atmospherical vicissitudes. Hence, the cause, to be thus wafted from one place to another, and to give rise to a particular disease, similar to that of the locality from whence it proceeds, must consist of a malarial poison, mixed with or suspended in the atmosphere.

"15. Equally favourable to the correctness of the belief, is the effect resulting from the upturning of soil in hot weather. The injury resulting from this operation, as illustrated by a wide diffusion of malarial fevers, and the great mortality often occurring on the levelling of streets, digging ditches and canals, cutting down bluffs, caving in of river banks, &c., cannot be accounted for on the principles advocated by the opponents of the malarial doctrine, and are more easily explained on the supposition of a poison exhaled from the decomposed organic matter contained in freshly exposed earth.

"16. The effect of partial draining; of exposing a virgin soil to the action of the sun; of the imperfect desiccation of the soil after an overflow, and other kindred occurrences, in producing or increasing fever; and the converse effect of complete draining, of covering a marshy surface with water or sand; or masses of decaying organic materials, or any other unhealthy place, in the same way, or with earth, in putting a stop to fever, lead to the same conclusions.

"17. Agreeably to no other doctrine than the malarial, can we explain the greater sickliness of marshes formed by the mixture of salt and fresh water; for this mixture cannot increase any of those influences to which others attribute diseases thus produced or aggravated; while we can understand that the mixture may act injuriously, by furthering the decomposition of the organic matter which it saturates, as well as by the reciprocal destructive action of fresh water on the living beings contained in sea water, and, reversely, the destruction by the latter of those contained in fresh water.

"18. The limitation of the disease to a restricted locality, to a few streets or buildings, to one house, or side of a house, or one room, &c., bespeaks, as do like occurrences on ship-board, the existence of a local cause of infection, and, consequently, the exhalation from this of a morbific agent.

"19. The opinion of fever being due to the introduction into the system of a morbid poison, floating in the atmosphere of sickly places, receives a strong support from the analogy existing between their symptoms and anatomical lesions, and those resulting from the introduction of putrid organic matter into the circulation, or the inspiration of putrid vapour.

"20. The arresting or mitigation of fevers in sickly localities by a resort to proper hygienic means, disinfectants, washing, excessive heat, &c., lead to the same opinion; for these means operate in the same manner on other morbific causes, the effluvial nature of which is undoubted, while they have no efficiency in cases of diseases arising from other agencies.

"21. The effects of trees, walls, hills, buildings, curtains, &c., in arresting the transit of the cause of fevers, are equally favourable to the idea of that cause being a malarial poisonous substance contained in the atmosphere.

"22. Still more conclusive is the destruction of that cause by frost, for the latter cannot produce its beneficial effects by an action on any of the other agencies to which fevers are ascribed; while it is known to exercise the same destructive influence over other causes of a character similar to the one from which fever originates.

"23. The wide prevalence of autumnal fevers in certain localities; the great mortality to which they sometimes give rise; and the diffusion of some forms of them over a very large expanse of country, at a time, too, when the sensible qualities of the atmosphere do not appear to have varied in any important point from what they are in healthy seasons, afford an additional reason for attributing them to a toxical agent floating in the atmosphere.

"24. The transmission of the disease to the fœtus in utero; its production from the internal use of the waters of marshes; the inability of such waters to sustain life in fish and other animals of the kind; the undermining effect of a malarial atmosphere on the system; the production by it of a state of cachexia, and its influence in shortening the duration of life, indicate the existence and agency of a poisonous substance transmissible, in the one case, like other morbid poisons, by the mother to the child she bears in her womb; in another case capable of solution in the water which helps to its generation: and in others, again, endowed, like well-known poisons, with the power of gradually occasioning peculiar and injurious changes in the blood and vital organs.

"25. The neutralizing influence exercised by the poisons of some zymotic diseases over the agent producing malarial fever, would seem to indicate the existence of a close analogy between the latter and the former. The same remarks are applicable to the pathogenic antagonism existing between malarial and typhoid fevers.

"26. Lastly, from all that we can gather respecting the origin, mode of progression, and phenomena of autumnal fevers, the nature of the localities they visit, the circumstances under which they appear, the agencies which promote their development, or retard or arrest their progress, we may conclude, without fear of error, that everything tends to connect the morbific agent, of which autumnal fever is the offspring, with the products of the decomposition of organic materials; requiring as it does, for its generation, the action of the very same agencies which are necessary for that decomposition. Like the latter, it requires the presence of the above materials; like the ordinary decomposition of these, the febrile cause requires for its generation a more or less prolonged continuance, and a certain degree of atmospheric heat; it requires, also, a certain amount of moisture. In the one as well as the other process, an excess of this moisture prevents or arrests its progress; in the one as well as the other, a total absence of the same produces a like preventive or destructive effect. The generation or diffusion of the febrile cause is promoted by a calm and close state of the atmosphere, and retarded, prevented, or modified by free ventilation, elevated situations, and a pure quality of the atmosphere. Like ordinary decomposition, the process by which the febrile cause is produced, is retarded by cold, and arrested by frost, as well as by intense heat."

D. F. C.